sures, the details of which will shortly be published, have led to the following conclusions in reference to the conditions necessary for the existence of any substance in the liquid state. These are two in number, viz.:—

1. In order to convert a gas into a liquid the *temperature* must be *below* a certain point (termed by Andrews the *critical temperature* of the substance), otherwise no amount of pressure is capable of liquefying the gas.

2. In order to convert a solid into a liquid the *pressure* must be *above* a certain point, which I propose to call the *critical pressure* of the substance, otherwise no amount of heat will melt the substance.

If the second of the above conditions be true, it follows that if the necessary temperature be attained, the liquefaction of the substance depends solely on the superincumbent pressure; so that if by any means we can keep the pressure on the substance below its critical pressure, no amount of heat will liquefy it, for in this case the solid substance passes directly into the state of gas, or in other words it sublimes without previous melting.

Having come to this conclusion, it was easily foreseen that if these ideas were correct it would be possible to have *solid ice at temperatures far above the ordinary melting-point*. After several unsuccessful attempts, I was so fortunate as to attain the most perfect success, and have obtained solid ice at temperatures so high that it was impossible to touch it without burning one's self. This result has been obtained many times and with the greatest ease, and not only so, but on one occasion a small quantity of water has frozen in a glass vessel which was so hot that it could not be touched by the hand without burning it. I have had ice a considerable length of time at temperatures far above the ordinary boiling-point, and even then it only sublimed away without any previous melting. These results were obtained by ***maintaining*** the superincumbent pressure below 4·6 mm. of mercury; ***i.e.***, the tension of aqueous vapour at the freezing-point of water. Other substances also exhibit these same phenomena, the most notable of which is mercuric chloride, for which latter the pressure need only be reduced to about 420 mm. On letting in the pressure the substance at once liquefies.

For the success of these experiments in the case of water one or two details of manipulation are necessary, but these will be subsequently described. THOS. CARNELLEY

Firth College, Sheffield, September 6

A Doubtful British Mollusk

I HAVE just observed that I am quoted in Dr. Gwyn Jeffreys' "British Conchology" (vol. v. p. 161) as an authority for the discovery of *Clausilia parvula* (a Continental snail) in Staffordshire. Many years ago, when I was a schoolboy, I found six or seven specimens at Kinver, near Stourbridge. I took them at first for a smooth variety of *C. rugosa*, but noting other differences sent them to Dr. Jeffreys, who identified them as above. I never had another opportunity of visiting the spot, but brother conchologists, who went on my recommendation, failed to find any specimens. The sheep-walk on which I picked them up was close to the grounds of Enville, where there are many foreign shrubs, and I have now little doubt that they were introduced. At the same time the large size of the specimens seemed to indicate that they had been long acclimatised, as northern individuals are larger than southern. GRANT ALLEN

Broad Street, Lyme Regis, Dorset

A Halo

MAY I mention a strange appearance which I saw in the heavens on August 29, and ask for an explanation of it? It was a rainbow without rain, and in the same quarter of the heavens as the sun.

At 5.45 p.m. I observed in a little nearly circular opening in the clouds, at the same height above the horizon as the sun, and about 23° to the north of it, all the colours of the rainbow. They were very vivid, and lasted for several minutes. Two persons who were with me also saw this strange sight, which I cannot account for in any way. Was it seen by any of your readers? And what could be the cause of it? L. SOAMES

Brighton, September 2

[This was probably a portion of the ordinary halo of 22°. If so, it indicates the presence of ice-crystals (not of drops of water) in the upper atmosphere. Such things are common enough in autumn, especially when there is a sudden lowering of temperature by an anticyclone.—ED.]

Tone of Violins

I SAW a little time back, but omitted to note it at the time, a brief notice of some German experiments showing that the strings of good old instruments of fine tone tended far more than in the case of inferior violins to vibrate in closed curves or simple curves. I have searched NATURE in vain for some weeks, but cannot find it, though I thought it was in these columns. I am particularly anxious to recover it for purposes of my own connected with another branch of physics, and shall be obliged if any reader can refer me to the notice, or to the paper, or any translation of it. LEWIS WRIGHT

August 30

ADOLPH EDOUARD GRUBE

BY the sudden death of Prof. Grube of Breslau on June 23, zoological science has been deprived of one of its enthusiastic and veteran cultivators. Born in Königsberg on May 12, 1812, he entered the university of that city in 1831, and graduated in medicine in 1837. Thereafter he became a private lecturer on zoology in Königsberg. In 1844 he was appointed to the Professorship of Zoology in the University of Dorpat, and lastly was transferred, in 1857, to a similar post in the University of Breslau, where he laboured till his death.

He chose for the subject of his inaugural dissertation (in 1837) the structure of *Pleione carunculata*, Pallas, and it is interesting that at this early age he selected one of the group in which his chief work in after-life was accomplished; for though he published various valuable researches in other departments (*e.g.*, those on the Branchiopod Crustaceans), still the Annelida most benefited by his labours during the subsequent forty-three years. Moreover, he observed so carefully, as well as laboured so industriously, that he was *facile princeps* in the department at his death. The bare enumeration indeed of his zoological works and papers is formidable; and their perusal bears imperishable witness to the well-directed energy and great ability of their author. He himself, with great modesty, used to state that his work fell far short of that of the late M. Claparède, who, with a delicate physique, nevertheless accomplished a marvellous amount of valuable work, both with pen and pencil. But though perhaps less of an artist than the talented Swiss, the greater tenacity of constitution in the stalwart German, combined with his indomitable energy and perseverance throughout a longer life, enabled him to overtake a much greater amount of work, especially in descriptive zoology.

The conscientious manner in which he carried on his scientific labours is well shown in his "Familien der Anneliden" (1851), a work which even now is of great value, and indispensable to workers in the department. The same may be said of his "Entwicklung der Anneliden" (1844) and his "Annulata Œrstediana" (1857). In his original papers in the *Archiv für Naturgeschichte* and in the recent admirable series in the *Sitzung der Schlesischen Gesellschaft*, on the families of the Annelida, he demonstrated the encyclopædian and critical knowledge which he had of the whole group in a remarkable manner, just as his "Bemerkungen über Anneliden der Pariser Museums" showed his great experience in discriminating the species described by others. His last large publication (a work of 300 pp., 4to, and fifteen fine plates by his tried assistant Assman) is devoted to the numerous Philippine annelids collected by Prof. Semper, and is a lasting memorial of his accuracy and patient industry.

Nor was he a zoologist who confined his researches to a single group. He was an accomplished carcinologist, and his faunistic treatises, *e.g.*, his "Actinien, Echinodermen u. Würmer des Adriatischen u. Mittelmeers," his "Ausflug nach Triest u. dem Quarnero," as well as his special papers on the Echinodermata, on *Peripatus* and other Arthropods, testify abundantly to the breadth

of his information and his unwearied efforts to advance zoological science. He was no less a thoughtful student of the labours of others than a discover of new forms and an accurate original inquirer.

To one who had worked at the fauna of Siberia, at the collections made during the Novara expedition and those of the German exploring ship *Gazelle*, at the varied stores in the "Museum Godeffroy" of Hamburg, who had made himself familiar with the shores of the Adriatic and the Mediterranean, as well as those of France and Britain, the splendid zoological series made by H.M.S. *Challenger*, under the direction of Sir Wyville Thomson and his colleagues, could not but prove an irresistible attraction; and it was this which tempted him more than anything else to make his last visit to this country in 1876, when he attended the Meeting of the British Association in Glasgow.

Privately Prof. Grube was one of the most amiable and accomplished of men. Of commanding presence (he was a cuirassier in his youth), and frank and manly bearing, his fund of general information, his musical tastes, and great geniality, endeared him to all his friends. Nor was he less *beloved* as a teacher by his students. Full of life and work, and with an industry that never seemed to flag, he was suddenly cut off in the midst of his labours, and just as he was organising fresh researches.

A full biography of Prof. Grube will appear in the *Leopoldina* in Halle, but, meanwhile, it is well to indicate in this country the sense of the great loss which zoological science has sustained by the death of this eminent investigator and teacher. W. C. M.

THUNDERSTORMS[1]

IV.

ALMOST all the facts to which I have now adverted point to water-substance, in some of its many forms, as at least one of the chief agents in thunderstorms. And when we think of other tremendous phenomena which are undoubtedly due to water, we shall have the less difficulty in believing it to be capable of producing thunderstorms also.

First of all let us think of some of the more obvious physical consequences of a fall of a mere tenth of an inch of rain. Suppose it to fall from the lowest mile of the atmosphere. An inch of rain is 5 lb. of water per square foot, and gives out on being condensed from vapour approximately 3,000 units of heat on the centigrade scale. The mass of the mile-high column of air a square foot in section is about 360 lb., and its specific heat about a quarter. Thus its temperature throughout would be raised by about 33° C., or 60° F. For one-tenth inch of rain, therefore, we should have a rise of temperature of the lowest mile of the atmosphere amounting to 3·3° C., quite enough to produce a very powerful ascending current. As the air ascends and expands it cools, and more vapour is precipitated, so that the ascending current is farther accelerated. The heat developed over one square foot of the earth's surface under these conditions is equivalent to work at the rate of a horse-power for twelve minutes. Over a square mile this would be ten million horse-power for half an hour. A fall of one-tenth of an inch of rain over the whole of Britain gives heat equivalent to the work of a million millions of horses for half an hour! Numbers like these are altogether beyond the limits of our understanding. They enable us, however, to see the full explanation of the energy of the most violent hurricanes in the simplest physical concomitants of the mere condensation of aqueous vapour.

I have already told you that the source of atmospheric electricity is as yet very uncertain. Yet it is so common and so prominent a phenomenon in many of its manifestations that there can be little doubt that innumerable attempts have been made to account for it. But when we consult the best treatises on meteorology we find it either evaded altogether or passed over with exceedingly scant references to evaporation or to vegetation. Not finding anything satisfactory in books, I have consulted able physicists, and some of the ablest of meteorologists, in all cases but one with the same negative result. I had, in fact, the feeling which every one must experience who attempts to lecture on a somewhat unfamiliar subject, that there *might* be much known about it which I had not been fortunate enough to meet with. Some years ago I was experimentally led to infer that mere *contact* of the particles of aqueous vapour with those of air, as they fly about and impinge according to the modern kinetic theory of gases, produced a separation of the two electricities, just as when zinc and copper are brought into contact the zinc becomes positively electrified and the copper negatively. Thus the electrification was supposed to be the result of chemical affinity. Let us suppose, then, that a particle of vapour, after impact on a particle of air, becomes electrified positively (I shall presently mention experiments in support of this supposition), and see what farther consequences will ensue when the vapour condenses. We do not know the mechanism of the precipitation of vapour as cloud, and we know only partially that of the agglomeration of cloud-particles into rain-drops; but of this we can be sure that, if the vapour-particles were originally electrified to any finite potential, the cloud-particles would be each at a potential enormously higher, and the rain-drops considerably higher still. For, as I have already told you, the potential of a free charged sphere is proportional directly to the quantity of electricity on it and inversely to its radius; so when eight equal and equally charged spheres unite into one sphere of double the radius, its potential is four times that of each of the separate spheres. The potential in a large sphere, so built up, is in fact directly proportional to its surface as compared with that of any one of the smaller equal spheres of which it is built.

Now, the number of particles of vapour which go to the formation of a single average rain-drop is expressed in billions of billions; so that the potential of the drop would be many thousands of billion times as great as that of a particle of vapour. On the very lowest estimate this would be incomparably greater than any potential we can hope to produce by means of electrical machines.

But this attempt at explanation of atmospheric electricity presents two formidable difficulties at the very outset.

1. How should the smaller cloud-particles ever unite if they be charged to such high potentials, which of course must produce intense repulsions between them?

2. Granting that, in spite of this, they do so unite, how are they separated from the mass of negatively electrified air in which they took their origin?

I think it is probable that the second objection is more imaginary than real, since there is no doubt that the diffusion of gases would speedily lead to a great spreading about of the negatively electrified particles of air from among the precipitated cloud-particles into the less highly electrified air surrounding the cloud. And if the surrounding air were equally electrified with that mixed with the cloud, there would be no electric force preventing gravity from doing its usual work. This objection, in fact, holds only for the *final* separation of the whole moisture from the oppositely electrified air; and gravity may be trusted to accomplish this. That gravity is an efficient agent in this separation is the opinion of Prof. Stokes. It must be observed that as soon as the charge on each of the drops in a cloud rises sufficiently, the electricity will pass by discharge to those which form the bounding layer of the cloud.

The first objection is at least partially met by the

[1] Abstract of a lecture, delivered in the City Hall, Glasgow, by Prof. Tait. Continued from p. 410.